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ABSTRACT

The value of transitional bilingual education (TBE) is discussed in the context of two competing theories of second language learning: (1) first language (L1) knowledge facilitates second language (L2) learning, and (2) the best way to learn English is to maximize time spent using it. First, the facilitation theory is examined in light of recent research and found to be flawed. Two significant national studies are shown to have findings contradicting it. The English time-on-task theory is then explored and found lacking, with evidence drawn from the same two studies. It is argued that the theory fails because it does not consider important mediating variables in program effectiveness. A third study is cited, one that suggests some variables explaining the success of TBE with less English-language time-on-task, including nature of time spent in an English language environment and psychological effects of providing some respite between exposures to the target language. Two new hypotheses are proposed, to be tested by research. The hypotheses are: (1) native language instruction should be minimal and used only in early instruction, and (2) teachers who are familiar with but not fluent in the child's native language are better teachers of limited-English-proficient students. In addition, a new experimental design is recommended. (MSE)

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BLINDED BY THEORY IN THE SEARCH FOR EFFECTIVE PROGRAMS FOR LEP STUDENTS: A CALL FOR TESTING NEW RESEARCH HYPOTHESES

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BLINDED BY THEORY IN THE SEARCH FOR EFFECTIVE PROGRAMS FOR LEP STUDENTS: A CALL FOR TESTING NEW RESEARCH HYPOTHESES

Central to the debate over how best to educate limited-English-proficient (LEP) children is the question of the effectiveness of transitional bilingual education (TBE) programs. TBE is native tongue, with English as a second language (ESL), instruction gradually transitioning over a three to seven year[1] period into all-English. TBE is probably the most commonly implemented, but most controversial, second language acquisition program in the U.S. and as a result there have been a number of extensive reviews of its effectiveness for LEP students (Troike, 1978; Engle, 1975; Baker and de Kanter, 1981; 1983a,b; Rossell and Ross, 1986; Rotberg 1982; Willig, 1985; Yates and Ortiz, 1983; Peterson, 1976; GAO, 1987; Holland, 1986; Ravitch, 1983; Dulay and Burt, 1978; Zirkel, 1972). None of these reviews, however, has provided a definitive answer to the effectiveness question. Indeed, we sometimes wonder, given the ideological fervor surrounding this issue and the limitations of social science research, whether a definitive answer can ever be provided to the satisfaction of both camps.

Although this paper's original intent was to update the most recent comprehensive reviews, Rossell and Ross (1986) and Holland (1986) conducted more than six years ago, we got side-tracked by an interesting issue inspired by two recent national research studies, Ramirez et al., 1991[2] and Burkheimer at al., 1989, and by the second author's evaluation of the Berkeley Unified School District's transitional bilingual education program (see Rossell 1990). All three of these studies are scientifically valid (most evaluations in bilingual education are not) in that

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[1] Although many states have laws limiting the amount of time in transitional bilingual education to three years, these laws are easily subverted by simply reclassifying the student but keeping the child physically in the bilingual classroom. Hence many students stay in TBE for their entire elementary career (seven years), although they may receive little or no native tongue instruction after reclassification.

[2] See our separate critiques of this study (Rossell, 1992; Baker, 1992) in the Bilingual Research Journal (formerly the NABE Journal), 16 (1992). The entire volume is devoted to the Ramirez study.

they included 1) two or more groups receiving alternative treatments with different amounts of English and native tongue instruction and 2) a statistical control for pretreatment differences when students are not randomly assigned to treatment groups.

We came to two initial conclusions in reviewing these studies. First, Ramirez et al. (1991) is a complex study whose findings have been largely misinterpreted, both by the study's authors and by the U.S. Department of Education, in an effort to make the empirical findings consistent with a questionable theory that guided the research design. Second, a consistent empirical finding of all three studies--Ramirez, et al. 1991, Burkheimer, et al., 1989, and Rossell, 1990--has been overlooked in part because it did not fit the theoretical perspective guiding each study. Before analyzing these three studies in more detail, however, we will discuss the theories that dominate policy thinking in bilingual education.

Second Language Learning Theories

Two competing theories of learning a second language lie at the center of the long debate over the value and use of transitional bilingual education programs in teaching English and other subject matter to language minority students in the United States. On one side are proponents of bilingual education programs who argue that children should be taught in their native tongue (L1) because there is a facilitating effect of the first language on second language (L2) learning. On the other side, critics of bilingual education programs put forth a time-on-task argument that the best way to learn English and subject matter that one will have to compete in in English is to maximize the time spent in English. Both of these competing theories have problems, although for different reasons, and there is empirical research that appears to contradict each. The facilitation effect theory is seriously flawed in its logic. The time-on-task theory is limited, a fact all too often ignored, by powerful constraints--1) school is not the only source of time-on-task, 2) there are important psychological and pedagogical variables that mediate time-on-task such that it is

effective or engaged time-on-task, not simple time-on-task, that influences achievement, and 3), a corollary of 2, for many if not most children there is a point in each day's instruction when there are diminishing marginal returns to further instruction in that subject. The problems with these theories are typically ignored, however, and virtually all bilingual education program evaluation studies are designed as a test of one of them in their simplest forms.

The Facilitation Theory

James Cummins (1978, 1981, 1985) is probably the principal proponent of the facilitation theory. Cummins' initial (1978) theoretical work addressed the problem of inconsistencies in the empirical research. Some studies had found initial instruction in the native tongue to be superior to immersion in the second language; most, however, had found it to be inferior. He set out to explain these conflicting findings. The facilitation theory he hypothesized had two components: 1) the "threshold" hypothesis which states that the cognitive and academic benefits of bilingualism are mediated by the levels of competence attained in L1 and L2--specifically there is a threshold level of linguistic competence which a bilingual child must attain in order to avoid cognitive disadvantages,[3] -- and 2) the "developmental interdependence" hypothesis that states that the development of skills in a second language is facilitated by skills already developed in the first language.

According to the first part of the facilitation theory, if bilingually taught children reach the threshold in their native language, they will attain a higher level in the second language than will students taught entirely in L2. On the other hand, if bilingually taught children have not reached the threshold in L1, they will be inferior in L2 (and L1) to students taught entirely in the second language. Thus, the conflicting research on TBE was

[3] The L2 part of this proposition is basically circular reasoning. In essence it says that a certain level of L2 competence is necessary to have a certain level of L2 competence. This part of the theory will thus be ignored in future discussions.

accounted for by whether or not the L1 threshold had been reached by the bilingual learners. Studies that found all-L2 instruction superior to transitional bilingual education, it was conjectured, must have had subjects in the bilingual education program who had not reached the threshold in L1, while studies where bilingual education was more effective than all-L2 instruction must have had subjects in the bilingual education program who had reached the threshold. An implication of this theory is that it takes considerable time before the threshold in L1 is attained and the facilitation effect is manifest. Thus, the longer a student stays in transitional bilingual education up to seven years, the better they will do in the second language because they have surpassed the threshold in L1 (and L2).

The second part of the facilitation theory claims that once a child has learned to read in his or her native language, learning literacy in the second language is facilitated because he or she has mastered the "mechanics"--the hardest part--in the easiest language and these mechanics will allow him or her to more easily acquire the second language. Since it takes 3-4 years to acquire literacy in roman alphabet languages,[4] the facilitating effect will not become apparent until 5-7 years after literacy instruction begins.

It is important to understand, however, that bilingual education programs in the U.S. (or in Canada) were not designed in response to the facilitation theory. It is the other way around. Although bilingual education programs officially began in 1968 with Title VII of the Secondary and Elementary Education Act, Title VII in fact was created in response to a civil rights movement whose leaders, upon observing that Hispanic children on average had lower achievement than white children, concluded that the cause of this must be the then

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[4] Perhaps one of the more serious flaws of the facilitation theory is its lack of attention to the hieroglyphic languages--that is, the Asian and Middle Eastern languages--that not only have no similarity to English in appearance, but take much longer to master. In other words, learning to read in L1 may actually be harder in these languages than in L2, if the latter is English or another roman alphabet language. As a result, we know of no non-roman alphabet bilingual programs in the U.S. that teach initial literacy in L1.

current practice of placing Hispanic LEP children in all-English regular classrooms and "forcing" them to give up their native tongue.

Bilingual education thus preceded the facilitation theory by more than a decade.[5] Indeed, the dominant element of second language learning theory in 1968 seems to have been the time-on-task principle. Izzo (1981) summarizes studies conducted in the late 60s and early 70s:

> The length of time spent in language study is, in fact, one of the most important factors in achievement...[Moreover] it must be the total length of time spent in contact with the language that is of importance in determining second language proficiency (Izzo, 1981:51,52).

Carroll (1976) goes even further in summarizing the Canadian research evaluations as "eloquent confirmation of the statement that time is the most important factor in learning [a second language]." These conclusions and theories dictated the practice of all-English instruction in the 1960s. Since all-English instruction had not eliminated the achievement differences between Hispanic and white children then it had to be replaced by its opposite--native tongue instruction--which would raise the self esteem and motivation of Hispanic LEP children and ultimately their achievement. Indeed, this is the kind of "civil rights" argument that dominated much of the educational reform movement of that era. The facilitation hypothesis was then created after the fact by Cummins and others to provide an educational/linguistic justification for a policy already implemented on civil rights grounds (see Rossell & Ross, 1986 for a discussion of the legal history).

Research and the Facilitation Hypothesis

Is the facilitation theory a valid theory? Much of the evidence Cummins cites to demonstrate its validity is either trivial--a study by Cummins and Mulcahy (1977) showing

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[5] Cummins cites a UNESCO study, The Use of the Vernacular Languages in Education. (Monographs on Fundamental Education, 1953) but there is no reference to this study in any of the legislation or literature of the 1960s.

that fluent bilingual Ukrainians did better than either non-fluent bilingual Ukrainians or monolingual students on a test of ambiguities in sentence structure[6], a study by Leslie (1977) showing that Indian children who scored high on oral Cree scored high on English reading,[7]--or just plain contrary--for example, a study by Hebert (1976) and another by Ramirez and Politzer (1976) showing that instruction in L1 has no effect on achievement in L2.[8]

The principal evidence Cummins cites for the effectiveness of transitional bilingual education programs, however, is Skutnabb-Kangas and Toukomaa (1976) who compared two groups of students who had immigrated from Finland to Sweden, those who immigrated before and those who immigrated after reaching the third grade. Students who immigrated after third grade, that is, those students who had been in school in Finland long enough to have first learned literacy in their native language (Finnish), supposedly performed better in Swedish than did the children who had moved to Sweden at a younger age and who presumably had begun learning Swedish at an earlier age. Both Cummins and Skutnabb-

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[6] Aside from the issue of the importance of the task, this study can tell us nothing about the kind of instruction LEP children should receive since the fluent Ukrainian bilingual students not only did better than the monolingual controls, but they also did better than the non-fluent Ukrainian bilingual students who were receiving identical instruction. It is probably the case that the fluent bilinguals were more intelligent to begin with since the definition of fluency, as it is with so many of these studies, is scoring high in both languages. Indeed, this apparent ignorance of the correlation between scoring high in two languages and native intelligence permeates the research on bilingualism.

[7] This only further underscores the extent to which these studies simply test native intelligence since there is no theory that argues high L1 oral fluency stimulates higher L2 reading levels.

[8] Although Cummins' theories are widely cited in the U.S. as evidence for the superiority of TBE, the research he relies on is research on bilingualism conducted in Canada, where the educational process of creating bilinguals is the reverse of the U.S. There, students are taught completely in L2 in kindergarten and first grade and gradually transitioned to mostly L1 by high school. Although everyone believes these programs to be quite successful, Cummins continues to be cited as evidence for the superiority of the U.S. version of bilingual education: L1 first gradually transitioning to L2.

Kangas/Toukomaa argue that the greater ability of these older students in Swedish is a function of more years of instruction in Finnish.

There are major methodological problems with Skutnabb-Kangas and Toukomaa, however, and with the inference that their results support the facilitation hypothesis. A detailed discussion of these problems can be found in Baker and de Kanter (1981), and Baker (1984), only three of which will we discuss here. First, Skutnabb-Kangas and Toukomaa reported no statistical analysis of their data. Baker and de Kanter (1981) conducted an extensive statistical reanalysis of the Skutnabb-Kangas and Toukomaa and found there was no greater relationship between length of schooling in the native land and later performance in the second language than could have been expected by chance.

Moreover, at the time of the Skutnabb-Kangas study, Swedish, the second official language of Finland, was a required subject in Finnish schools from the third grade on, a fact which neither Skutnabb-Kangas/Toukomaa nor Cummins mentions. Thus, if the simple descriptive data presented by Skutnabb-Kangas and Toukomaa show anything, it is that students who have a chance to study a second language before immigrating to the country of that language perform better in it than do students who have had no formal instruction in it before they immigrated. In short, contrary to Cummins' assertions, there is no empirical support in Skutnabb-Kangas and Toukomaa for the facilitation hypothesis and some support for the time on task principle.

There is other research which is sometimes misinterpreted as support for the facilitation hypothesis. This research shows: 1) children can transfer skills learned in one language to another language (Lambert and Tucker, 1972; Barik and Swain, 1975; Bruck, Lambert, and Tucker, 1977) and 2) older children are, contrary to popular belief, more "efficient" (i.e. faster) learners of languages (Ausubel, 1964; Taylor, 1974; Ervin-Tripp, 1974; Stern, Burstall, and Harley, 1975; Eckstrand, L.H., 1975; Ramirez and Politzer, 1978; Swain, 1981). Hence, when it is observed that older learners who already knew how to read in their

native tongue acquired a second language faster than younger learners, some individuals have interpreted this as support for the facilitation effect (see, for example, Rosier and Farella, 1976; Rosier and Holm, 1980; Skutnabb-Kangas and Toukomaa, 1976). When the proper analysis is conducted, however, the causal variable turns out to be age, not native tongue reading ability (see Izzo, 1981).

Indeed, Baker and de Kanter (1981) noted that while there were a number of methodologically sound studies that showed bilingual education programs to be superior to all English instruction, none of these studies supported the facilitation hypothesis because they did not find evidence of a long term, delayed effect on L2 literacy from enhancing L1. Rather, the few effective bilingual education programs seemed to show an early superiority to all-English instruction, something that according to theory, should not happen. Since this finding was contrary to the widely accepted theory, Baker and de Kanter did not pursue its implications beyond noting that something was amiss, and there was, therefore, no justification for a bilingual education program mandate.

Collier (1987a, 1987b) has conducted one of the few studies that directly attempts to test Cummins hypotheses. Collier's data, however, contradicts the theory she purports to have proved. The underlying hypothesis, derived from Cummins, that Collier tested is that there is a facilitation effect of L1 on L2 and therefore, short LOR (Length of Residence) students will score higher on L2 than longer LOR students. Collier presented 20 pseudo-learning curves derived from cross-sectional data, which she incorrectly describes as "rates" of learning.[9] If the facilitation hypothesis is correct, these curves will be negatively accelerated--that is the shorter the length of residence in the U.S., the higher the achievement in English. Of the 20 curves, however, only 2 (or perhaps 3) clearly show negative acceleration. About 8 curves show positive acceleration. If one simplifies the

[9]Since they are cross-sectional, not longitudinal data, she cannot show rates of learning only levels of achievement at a particular point in time.

problem of interpreting the curves by asking only if the two end points of the curve show negative acceleration, there are 8 that support the hypothesis and 12 that contradict it.

In addition, there is a second way in which Collier's results contradict Cummins. Collier claimed evidence of the facilitation effect for children aged 8-11 and argued at great length as to why the facilitation effect should not be found in older children, where her data most clearly contradicts Cummins. Cummins (1981a, 1981b) however, argues that the facilitation effect is found in children aged 12-16. We must conclude, therefore, that Collier's data, rather than supporting Cummins' position, show that Cummins is wrong. Indeed, Collier herself recognized this because she devoted extensive space in her article to arguing that we should ignore her negative data. Thus, one of the few researchers to directly test Cummins' theory finds contrary evidence, but because of the importance of the facilitation effect for transitional bilingual education, urges us to discount her findings instead.

Several other researchers have directly or indirectly tested the facilitation effect. The Eastman Project (Krashen and Biber, 1988) is perhaps the major effort to demonstrate the facilitation hypothesis. Although Krashen and Biber claim to support the facilitation hypothesis, their analysis is so severely flawed that no conclusions can be drawn from it other than that the Eastman program clearly provides no support for the facilitation hypothesis (see Baker, 1990 for a critique of Krashen and Biber).

In addition to the lack of empirical support for the facilitation hypothesis, it is a poorly cast theory. There is no underlying psychological mechanism that accounts for the facilitation effect. Rather than being deduced from well established mental processes, the facilitation effect has to be accepted as a fundamental characteristic of the brain itself. At least parts of it, if not the whole hypothesis, are untestable. It can be determined if the L1-L2 sequence is superior to simultaneous teaching of L1 and L2 or to L2 alone, but if it should be shown that L1-L2 is superior, it is impossible to say why. That is, exactly what

knowledge transfers from L1 to L2? Is it, for example, the concept of a sentence? This is unknowable because all literates, be they monolingual or bilingual know what a sentence is. Is it the process of decoding? Why and how do we find this out? In short, we simply do not know what particular mental process or items of literacy related knowledge possessed by a monolingual facilitates his or her learning a second language.

This is an important methodological point which we encounter elsewhere in other forms. An overly obvious example should make it clear. Imagine a study that set out to identify the characteristics of a good teacher and found that all good teachers erase the blackboard (this is not just imaginary; all good teachers do erase the black board). It does not follow from this confirmed empirical fact that the way to improve achievement is to direct all teachers to spend all day erasing the blackboard. Likewise, if the L1-L2 sequence is superior, nothing follows about what particular components of L1 accounted for the superiority of that method and there is nothing in the literature that tells us. Indeed, the drawings that often accompany Cummins' articles show empty heads with no more than two descriptive labels and no discussion whatsoever of any mental processes involved in the theory he advocates.

Perhaps the most important recent test of Cummins' facilitation theory is the Ramirez et al., 1991 national study. Although not much discussed in the final report, its design was specifically structured to test Cummins' facilitation theory.[10] The study sampled early-exit TBE classrooms, immersion (all-English special instruction), and late-exit bilingual (maintenance) classrooms across the U.S. Only the early-exit and immersion classrooms were directly compared to each other in statistical analysis. The third grade cohort was included in the design in order to create a synthetic cohort estimate, by combining the late exit kindergarten and third grade cohorts. Learning curves over time

[10] The first author of this paper was the project officer at the Department of Education for the Ramirez et al., study.

were made a central part of the analysis to test both Cummins' theory and the alternative, time-on-task theory that predicts immediate and continuing superiority for all-English programs.

In the early-exit and immersion program comparisons, Ramirez, et al. found over two years (kindergarten to grade one) a significant effect favoring early exit bilingual education programs in reading, but not in language or math where there was no difference between programs. This advantage of bilingual instruction, however, had vanished by the end of four years of schooling. Now structured immersion was favored in language arts, but not in math or reading where there was no significant difference between programs.

One of the greatest failings of the study is that it did not statistically compare the late exit programs directly to the early exit and immersion programs, but tried to do so indirectly with simple descriptive line graphs. The theoretical framework of the study--a comparison of the facilitation and the time-on-task hypothesis--led Ramirez et al. to misinterpret the merely descriptive line graphs (TAMP curves)[11] and to overlook a finding with stronger empirical support which fit neither theory. Ramirez et al. concludes that late exit programs have a more positive effect than early exit or immersion programs because they have a positive curvature over time; that is, the rate of gain increases from year to year. However, the report's conclusion that this difference in curvature represents an advantage for late exit programs does not necessarily follow. Late exit students may be playing catch-up. They appear to have fallen well behind their immersion and early exit peers through the third grade, and it is indeed fortunate for them that they do begin to catch up later on. Unfortunately, the data available does not make it possible to determine

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[11] The descriptive TAMP curves are invalid because there is no control for student and other characteristics, as Ramirez et al. admits earlier in the report. See Rossell (1992), Baker (1992), and National Research Council (1992) for a discussion of these and other problems. See Collier (1992) for a complete failure to understand that one can draw no conclusions from these curves.

if they ever actually overcome the handicap they appear to have acquired during the first three years of the late exit program.

While there are enough problems with this study that it should be interpreted with great caution,[12] the fact that the early exit program did as well as it did in comparison to immersion suggests the following possibility: bilingual education may be superior to all-English instruction in the very beginning when a student literally knows no English, but as the student's English language knowledge increases and English becomes more comprehensible, time on task in English becomes more important because it becomes effective time on task. Ramirez et al., on the other hand, shows no support for the facilitation effect since in that study and contrary to the exaggerated claims made, the descriptive portion of the study--the TAMP curves--show that students who stayed in bilingual education the longest did the worst. While this apparent negative finding may not be real given the lack of statistical control for student and classroom characteristics, it is definitively not positive evidence.

The Burkheimer, et al. (1989) study, also funded by the Department of Education and criticized by the National Research Council (1992), shows similar findings. Not only is there no facilitation effect, but the only positive effect for bilingual education in English language arts and math was found in the very beginning of a student's English language acquisition. Although overall, more instruction in English and less in Spanish language arts was positively related to increased English language arts achievement over a one-year period, this varied by initial English proficiency. First graders whose proficiency in English was sufficiently high (not defined) had an increase in achievement when there was

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[12] One of the two greatest mistakes of the Ramirez et al. 1991 study was the use of nominal program designation as the treatment variable rather than the percentage of English used in instruction, which varied considerably within nominal program categories and by subject matter. The second greatest mistake, as mentioned above, was the failure to directly compare with statistical analyses the late-exit program to the early-exit and immersion programs. See Rossell (1992) and National Research Council (1992).

more instruction in English. Those who were less proficient in English initially, had greater end of year achievement with more hours of native language arts instruction and more English language arts in the native language (i.e. using the native tongue for assistance in English instruction) (Burkheimer, 1989: 5.43).

Overall, more instruction in English was associated with higher math achievement and this effect was greatest for those who had higher math scores on the math pretest. In the nominal second grade, however, children achieved higher in math if they received most of their math in Spanish, provided they were given sufficient instruction in English language arts. If mathematics was taught primarily in Spanish, everything else needed to be taught in English in order for the child to do well in math. This was not true for third grade children, however. For these children, Burkheimer et al. found an "unqualified positive contribution from greater instruction in English language." He concludes, "For success in mathematics, there is an increased requirement for proficiency in English" (Burkheimer, 1989: 6.27). This is, however, contrary to the facilitation theory.

Instruction in ethnic heritage decreased both the overall effect of more English language arts and the relative advantage for higher pre-achievers. This is because as Burkheimer notes--but which is seldom acknowledged in this field--

> ...within a framework that constrains total instructional hours and is further constrained by legislated requirements for some courses, increased instruction in one particular subject area is typically accomplished at the expense of reduction in another subject area...As examples: maintenance of the child's native language skills is accomplished at the expense of reduction in another area; more hours devoted to Ethnic Heritage instruction reduced the remaining hours that could be devoted to other subjects. This reality is not a value judgement of what should or should not be taught (which is best determined by local conditions and goals), but simply a recognition that trade-offs are required (Burkheimer, 1989: 5.42-43).

Finally, the Burkheimer study found a seemingly contradictory finding: having a bilingual certificated teacher had a negative effect on English language arts achievement

for the first grade cohort, but familiarity with the child's native language had a positive effect. It is not clear what this means, but if we assume that being bilingual certified means a more advanced knowledge of the native tongue than not being bilingual certified, it may mean that some native tongue ability is good, but too much is not. This hypothesis is suggested by other research as well. Fillmore (1980), for example, examined different kinds of bilingual education classes and found that the teacher who was most successful in raising the English language achievement of her Chinese students knew the children's native tongue, but taught 90 percent in English. Similarly, the Austin Independent School District's TBE program (Carsrud and Curtis, 1980) found to be superior to submersion, had teachers who used English as the medium of instruction 82 percent of the time.

Tickunoff's (1983) descriptive study of successful bilingual instruction (58 teachers from 6 nationally representative sites) identified the following characteristics of successful programs: 1) 80 percent of time allocated to academic learning tasks, 2) L1 used by teachers primarily to clarify instructions, and 3) content areas such as math and social studies taught in English. Two studies of the achievement gains of LEP children taught by bilingual and monolingual teachers (Ligon, 1974; Curtis, 1984) found no difference between the two. Similarly, the AIR (Danoff, et al. 1977; 1978) national survey of bilingual education also found no relationship between whether a teacher was bilingual and the performance of his/her students. Rossell (1990) found no difference in achievement between whether a teacher was bilingual certified and his or her student's achievement.

Even more amazing, Moore and Parr (1978) found teachers in the bilingual education program who were rated as less competent had better student performance. This finding is not as strange as it sounds if, as seems likely for a bilingual education program, the competence rating is primarily an evaluation of the teacher's bilingual ability. What all these studies suggest is that the psychological and perhaps initial pedagogical advantage one may gain from having a fluent bilingual, same ethnic group teacher may be offset by the

tendency of such teachers to teach too much and too long in the native tongue--in other words, to teach according to the facilitation theory.

All English Time-on-Task

Opponents of bilingual education programs argue that learning English is determined almost entirely by the time spent studying English (see Porter, 1990a, for the most recent example of this). The theory proposes that bilingual education programs are inferior to all English instruction because bilingual education programs reduce the time spent on the task of learning English.

The time-on-task hypothesis, like the facilitation theory, has a major problem in its failure to address the inconsistent research results. The time-on-task hypothesis requires the superiority of all-English instruction in every case, but the literature does not show this. If time on task were as important as its proponents suggest, all-English instruction would always be superior to any form of bilingual education, even programs where the native tongue is used only in small amounts, and monolingual English teachers would consistently produce greater achievement in their students than bilingual teachers. The scientifically valid studies cited above indicate, however, that this is not the case.

Any acceptable theory of teaching English to LEPs must account for the contradictory research evidence, as Cummins tried, but failed to do, with bilingual education. Why is it that bilingual education programs with up to 30 percent native tongue instruction often do no worse than and sometimes do better than all-English programs? Why is it that monolingual teachers do no better than bilingual teachers?

The problem with the time on task theory is that it fails to take into account important mediating variables. Although researchers such as Wiley (1976), Rosenshine (1979), Clauset and Gaynor (1980) find time on task to be the single greatest predictor of achievement in a subject, it is nevertheless only one of many instructional factors such as

classroom atmosphere, pace of instruction, and curriculum content that influence academic achievement. Karweit (1983) in a review of the time on task literature, concluded that the time on task effect, while significant, had been greatly overblown in importance. In reanalyzing the Beginning Teacher Evaluation Study (Fisher et al. 1980), Karweit estimates that an additional 60 minutes per day in time allocated to reading comprehension alone would be needed to increase reading comprehension scores by .25 standard deviations. If that were engaged time on task, however, only a 10 minute increase would be required to increase mathematics achievement by .25 standard deviations.

Rossell and Ross (1986) suggest some possible mediating factors for time on task to explain why many methodologically sound research studies show TBE with less English language time on task to be no different from or superior to submersion with more English language time on task.

> One possible explanation for this lies in the nature of the time spent in an English language environment in each alternative. Since much of the learning in a submersion situation is, at least initially, not effective learning because the students do not understand what is going on, a bilingual program which gives the children half of their education in English, but structures the English so that it is understandable may provide more effective time in the English language in the beginning than a program which is completely in English, but only a small part of it is comprehensible. As English becomes more understandable over time, the greater time spent on English in the submersion situation would give these children an advantage (See also Krashen, S., 1980: Long, M., 1981; Swain, M., 1983) if the bilingual program were still truly bilingual. Transitional bilingual education programs, however, usually reduce the native tongue and increase English over time, so that at the end of three years, students in both the submersion situation and bilingual education may end up with the same amount of effective learning time in the English language. This may be one reason for a common finding of the research -- no significant difference between doing nothing and transitional bilingual education.
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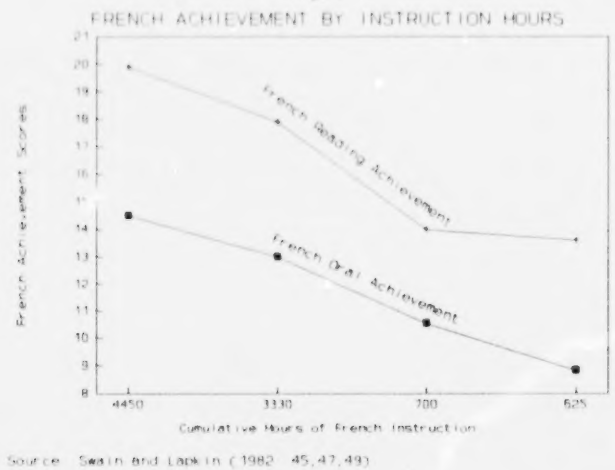
> Another possible explanation for the frequent lack of harm, and sometime benefit, of transitional bilingual education is that the program has important psychological effects which compensate for the reduced English language learning time. That is to say, if students in submersion programs often feel alienated or inferior, and if a special program regardless of its educational utility makes school more enjoyable, then they may come to school more often and stay longer. If in the submersion situation, they were taught 100 percent in English, but only came to school 75 percent of the time and only half of

> that was comprehensible in the first year, they will have less effective English language learning time for that year than if they had been in a bilingual program which taught them 50 percent in comprehensible English, but motivated them to come to school 85 percent of the time (Rossell and Ross, 1986: 407).

Porter (1990a: 125), by contrast, says "Education research, we have seen, makes the compelling case for time-on-task as the most important single determinant of success in learning *anything*" [emphasis in the original]. Porter provides two justifications for her sole reliance on the time-on-task principle. The first is her own experience running a mostly English "bilingual" program in Newton. But her assertion that the Newton program is superior to TBE is unsubstantiated by empirical evidence such as test scores, dropout rates, or other school success indicators. Indeed Porter offers only her personal opinion that she is right and the bilingual education advocates are wrong, resting her claim solely on the validity of the time on task principle.

Porter's second justification for her assertion that "the more time spent learning a language, the better you do in it" is the results of the Canadian immersion studies. It is true that these studies show an extraordinarily strong relationship between time on task and achievement as shown in Figure 1. But we would not expect the relationship between French instruction and French

Figure 1



Source: Swain and Lapkin (1982: 45,47,49)

nch instruction to be this strong in a community in which French was spoken outside the school. It is only because the Canadian immersion schools are the sole source of French for the English dominant students who attend them that the relationship is as strong as it is. The relationship between the extent of English instruction and English language achievement, for example, is not going to be as strong in the U.S. (nor was it in Canada) because students hear and speak English outside the classroom. Since we cannot control for that, the time on task relationship will be attenuated in comparison to French instruction in the Canadian studies.

Rossell's (1990) comparison in Berkeley of TBE and all-English regular classroom instruction with ESL pull-out has some interesting findings that are relevant to this discussion. First, she found that there was no difference between the two programs in the first year's analysis. Interviews with teachers indicated that they used English about 50-75 percent of the time in kindergarten and first grade and not much after that except for individualized instruction with new NEP students who entered in the later grades. One bilingual program in Berkeley did better than all others, including the regular classroom--ESL pull-out program. This school was specifically cited in the original court complaint as using too much English in its bilingual program (although it did have some native tongue instruction).

The second year evaluation occurred after the Berkeley Unified School District had increased the use of Spanish in its bilingual program in response to pressure from the California State Department of Education. This analysis found the bilingual program to be worse than all-English instruction by about 12-15 points in reading, language, and math. The first year findings, in comparison to the second year's, suggest one of two possibilities: 1) there is some threshold below which native tongue instruction does not harm children, or 2) there is some initial period of time when native tongue instruction actually benefits

students. We do not have enough evidence at the moment to confirm or rule out either possibility, but we can theorize about what is at work here.

A well established principle in learning theory is the differential effects of massed verses spaced learning trials. One classic demonstration of the difference is Duncan (1951) who studied the acquisition of the skill of keeping a pen point on a moving target. One group of subjects practiced for the entire learning time allocated. The other group was periodically interrupted for rest periods so that they were resting for two thirds of the whole practice session. The group with less practice learned better. Kientzle (1946) found that printing upside down was learned better with a rest of seven days between trials than with continuous practice or with as much as a 20 second rest between trials. Lyon (1917) looked at the average length of time it took to memorize a nonsense syllable in lists of different lengths comparing continuous practice and once a day practice. For a list of 40 nonsense syllables, the average learning time per syllable was four times longer for continuous practice than when the list was practiced just once a day.

The probable explanation for the superiority of spaced practice over continuous practice is that it takes time for the memory process to work. A constant barrage of material to learn overloads the memory process and interferes with learning. Rest, or doing something else, between practice sessions gives the memory process the time it needs to operate thus resulting in more efficient learning results.

Although learning a language is not exactly the same process as learning a boring, repetitive task, there is enough similarity that these studies are suggestive of what might go on in language learning. Consider language learning for the school aged, English monolingual child. The child already knows most of the words the teacher uses on any particular day. The few new words to be learned are interspersed with periods of no learning of new words -- rest. Therefore, language development in the child takes place in a setting of spaced, not continuous, practice.

However, the situation is initially quite different for the child learning a second language. Since all words are new at first, the situation is one of continuous learning. The student may actually make more progress learning the second language if rest is introduced into the constant stream of exposure to the unknown language.

How can rest occur in exposure to a new language? One, although by no means the only, way is to change the language of instruction to one the child already knows. Eventually, enough of the second language will be learned if it is part of the instructional program[13] so that the second language learner will be able to get some "rest" between new words in an all-English instruction.

Thus, it is possible that bilingual education programs, because they provide a needed rest from constant exposure to the new language, can produce better learning at the early stages of learning a second language. Later on, however, instruction entirely in the second language works as well as or better than bilingual education when English is comprehensible enough so that new words are a minority.

Conclusions

More than 15 years of research and literally thousands of studies since Cummins first proposed the facilitation theory have confirmed neither the theory nor the predicted effectiveness of bilingual education programs. Even advocates of TBE have had to admit this. Hakuta, in his 1986 review, Mirror of Language concludes:

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[13] Because of the influence of the facilitation theory, however,it is not always the case that English will be part of the instructional program. The second author has personally toured dozens of kindergarten, and a smaller number of first grade, classrooms in Massachusetts where almost no English at all is used in instruction. The teachers justify this on the grounds that their students need a long time to develop a high level of native tongue proficiency--a prerequisite to future academic success in the all-English classroom. In short, they are believers in the facilitation theory even if they may not know its name.

> There is a sober truth that even the ardent advocate of bilingual education would not deny. Evaluation studies of the effectiveness of bilingual education in improving either English or math scores have not been overwhelmingly in favor of bilingual education...An awkward tension blankets the lack of empirical demonstration of the success of bilingual education programs. Someone promised bacon, but it's not there (Hakuta 1986: 219).

Carter, a longtime advocate of bilingual education begins a 1986 article with the question: "Regardless of the many roots of the debate, one issue is unresolved. Does bilingual education work?" (Carter, 1986: 201). Paulston, a linguist and advocate of bilingual education, makes some telling points in her 1982 report to the National Swedish Board of Education. With regard to transitional bilingual education in the U.S. as the best way to learn English, she notes:

> The rationale for bilingual programs are that they are more efficient in teaching English although there is not much hard data to support such a view; it has however been the standard argument...The Canadians believe, with justification, [emphasis added] that fluent proficiency in the target language only occurs when that language is used as a medium of instruction (Paulston, 1982: 47-48).

She also approvingly cites Toukamaa who writes:

> ...we wish to dissociate ourselves from those arguments for teaching in the mother tongue, which attempt to frighten parents into choosing mother tongue-teaching by threatening emotional and intellectual under-development in those children who do not receive mother tongue-teaching. Teaching in the mother tongue does not seem to have the magical effect on the child's development, for good or for ill, which it has sometimes been ascribed (Toukamaa, 1982 :103 in Paulston, 1982: 49).

The Association for Supervision and Curriculum Development, an organization of 90,000 principals, school superintendents, teachers and other educational leaders, noted in their April 1987 report on bilingual education that "It is unclear which approach is better [teaching children in English or in their native tongue]" (Association for Supervision and Curriculum Development, 1987: 35).[14] Unfortunately, the latest

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[14] Despite these doubts expressed in several places in the report, they go on to recommend native tongue instruction.

scientifically designed research project, the Ramirez et al. 1991 study costing millions of dollars, has made only a small contribution to our understanding of this issue.

Thus, the research continues to be as inconsistent as it was when Cummins in 1978 first tried to account for its inconsistencies. Nevertheless, the facilitation hypothesis has been overwhelmingly accepted by bilingual educators as a proven fact and as the reason why TBE is superior to all other forms of second language acquisition techniques, despite the lack of evidence that superior ability in L2 results from reaching a certain threshold level of ability in L1. Indeed, the Ramirez et al. (1991) and Burkheimer et al. (1989) studies suggest to us that the threshold theory may work the reverse of the way that it is hypothesized to do so by the bilingual education advocates. It seems more likely that a threshold in L2, not L1, skills needs to be passed before L2 instruction is superior to L1 instruction. Indeed, L1 skills (after controlling for intelligence, something almost no one does) could be <u>irrelevant</u> to this process.

The research suggests this is a real possibility that needs to be investigated. Assuming that the goal of bilingual education in the U.S. is to make LEP students successful in English and in an English language environment,[15] we suggest two new hypotheses to be tested by research:

> 1) Native tongue instruction should be minimal and used only in the beginning when a LEP student's English language knowledge is very low;
>
> > a) the time period for the superiority of or need for native tongue instruction may be a matter of <u>months</u>;
>
> 2) Teachers who are familiar with, but not fluent in, the child's native tongue are better teachers of LEP students than fluent native tongue speakers because the former will emphasize

[15] This is, of course, not the goal of all advocates of bilingual education. For example, a bilingual maintenance program whose goal was the highest level of skills in two languages regardless of the cost to either, would look very different from a transitional bilingual program. See Rossell and Ross (1986) and Rossell (1990) for a description of the characteristics of an ideal bilingual maintenance program.

transition to English, and the latter will emphasize maintenance of L1 and development of L1 language arts which ultimately will work to the detriment of academic achievement in English.

We must emphasize that these are not conclusions. There is not enough reliable or consistent evidence to generate anything but hypotheses. If these are accepted as viable hypotheses, however, researchers would have to design very different research studies from the ones we have seen in the past. A research study to test these hypotheses would look something like this:

1. Treatment Groups: Truly LEP[16] students in classrooms categorized not by nominal program type but by the amount of English language instruction by subject matter and the varying credentials and native tongue ability of the teachers and aides;

2. Covariates: Student background characteristics, including use of English at home and neighborhood, intelligence (nonverbal such as Raven's Progressive Matrices), and prior English language ability level and program.

3. Outcome variables: Academic achievement by subject matter (as well as attitudes, drop out rates, etc.) measured every three months during the academic year, not simply at the beginning and end of the year,

It is important that outcome variables be measured in months, not years. As we have suggested, English may become sufficiently comprehensible to most or many LEP students sometime during the first year to make the positive time on task in English in the all-English classroom a better situation for the latter, but not the first, part of the year, hence the no difference finding in many scientifically valid studies.

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[16] The procedures currently used consistently overidentify LEP students. See Rossell and Baker (1988) for a lengthy discussion of how and why this happens and Rossell (1992) for a discussion of how to avoid inadvertently analyzing the effect of second language acquisition techniques on students who are not really LEP.

Bilingual programs that transition to all or mostly English midyear may be the ones found to be superior in the scientifically valid studies.[17] When the bilingual program attempts to maintain the native tongue in order to reach a threshold of L1, it could become the inferior program because L1 is introduced into a fixed school day where the maintenance of native tongue means less instruction in English over time which for many students means less achievement in English and/or subject matter tested in English.

These are speculations that we should by now have the answers to, but do not, perhaps in part because bilingual education research has been dominated by a fatally flawed theory--facilitation--and a simplistic theory--time on task. Both of these ignore the possibility of an early developmental period in which native tongue instruction may be superior when it is used, not to develop L1, but for assistance, or to give LEP students a rest so English instruction can be absorbed. Another possibility is that native tongue instruction may simply be "not harmful" for many or most non-English-speaking students perhaps because when English is not at all or barely comprehensible, it may not matter what you do instructionally after a student's attention or comprehension span has been exceeded.

Our discussion of these issues has been limited to the American educational system in which all-English regular instruction with ESL pull-out is the alternative to transitional bilingual education. It may be that the ideal program is "structured immersion" modeled after the Canadian immersion programs where the entire classroom consists of LEP students, the pace of instruction is structured to their level, and instructors teach

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[17] Baker and de Kanter (1981) found in L2 learning TBE was superior to submersion in 33% of scientifically valid studies; inferior in 17%, and no different in 50% In math, 14% found TBE to be superior, 21% found it inferior, and 64% found it to be no different from submersion. Rossell and Ross (1986) found in L2 learning that TBE was superior to submersion in 29% of scientifically valid studies, inferior in 21%, and no different in 50% In math, 7% of scientifically valid studies showed TBE to be superior, 27% showed it to be inferior, and 67% showed it to be no different from submersion.

completely in L2, although they know the child's native tongue.[18] Nevertheless, it cannot be emphasized enough that the research clearly shows, as with all other educational interventions, that the intervention itself is the least important factor explaining achievement. The most important factors in a child's acquisition of English and other subjects are the child's family characteristics, his or her intelligence, the characteristics of his or her classmates, and the intelligence and talent of their teacher. For most students, at least in an educational system in which all programs provide substantial amounts of English, the exact percentage of each language has, on average, explained only a small portion of the variance in achievement (see Burkheimer et al., 1989). Even in the worst cases we are struck by how small the differences in academic achievement are--a maximum of about 15 points--between programs with very different amounts of English instruction, although for any single student there could be serious consequences.

If we are ever going to come to a conclusion on this issue, we will need the courage and cooperation of school administrators in not only agreeing to studies such as the above, but to <u>random</u> assignment to alternative language instruction programs, thus eliminating the need for a pretest--one of the banes of research in this field. While federal and much state law forbids refusing special help to limited English proficient children, it does not prohibit placing them in alternative special programs. Given the inconsistency in the research, it seems to us that we have reached the point where it would not be unethical to randomly assign students to alternative treatments. In fact, we believe that the ethical position is to determine what is in the best interest of LEP students. Random assignment would go a long way toward producing a research study that could do this.

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[18] Eventually the Canadian programs become bilingual after the students have learned French in an immersion situation. The purpose of the transition from immersion to bilingual education around the second grade is to maintain the native tongue (English)--a necessity in an English dominant country.

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